

APPENDIX IV: COMMENTS AND RESPONSES TO CO-MANAGERS' DRAFT

17 May 2004

Dr. Peter Lawson
ONCC-TRT Co-chair
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Dear Pete:

Thank you for the opportunity to review “Identification of Historical populations of coho salmon...in the Oregon Coast Evolutionary Significant Unit”. This is a well-researched document that should be useful for your recovery planning.

In general I found the report to be scholarly, well-grounded theoretically, and clearly written. I offer the following comments constructively; I suspect that they are things that have already been considered by the TRT.

- 1 | 1. One of my main problems in thinking about the ESA listing or delisting process is with the ESU concept. Theoretically it can make sense, but in practice it is very difficult to define precisely. This confounds things then when it comes to classification of populations into your three categories. For example, if a population is “Functionally Independent”, then why is it not an independent ESU since it would be a “distinct population segment”?

I appreciate that you do not rely just on allozyme or DNA analyses. With these, unfortunately, we can only look at such a small, perhaps uninformative, part of the genome.

- 2 | 2. The assumptions in the report appear reasonable. It would be useful to include some justification for some. For example, why is using 100 year’s persistence as a criterion (page 10) appropriate. Would it change things if the criterion were 150 or 200 years? Likely not.

- 3 | Similarly, how precise is the break-point on figures 20 and 21 [new figure numbers 19 and 20] allowing separation of persistent from non-persistent populations. Are the data in these figures really best described by more than one model. In other words, do populations in “miles of high quality habitat” play by dissimilar ecological rules than those in systems with few mile of good habitat? Hence, if so, the data should not be thought of as continuous. Perhaps there two models that could best describe the data and their intersections best identify the breakpoint.

- 4 | It would also be good if justification for using 95% proportion native return rate could be provided. Would any classification change if 90% or 99% were used?

- 5 | I think that the important thing would be to use values for criteria that are conservative; i.e., sufficiently large so that any conclusion would not result in an error relative to fish protection.

6 | Given that the conclusions are based on multiple assumptions, has any analysis been done about the potential for compounding errors? In other words, if each assumption had only a small probability of being wrong and if several of these were indeed wrong, then the confidence in resultant conclusions could be quite low since the probability of being wrong for each would be multiplicative in total.

7 | 3. I agree with the decision not to use size as a criterion for smolts. One question would be if the timing of smolt migrants are based on in-river migrant traps, how far upstream are those traps and are these fish really headed straight out to sea from the lower river or do they “hang out”, perhaps in the estuary.

8 | 4. The figures of spawning timing are not intuitive. Are these the means of spawn timing of estimates over time? Is there some variation associated with these data?

Again, I appreciate the chance to look at this document. I suspect that the synthesis presented will be useful by agencies far beyond federal listing issues.

Cheers,

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April 26, 2004

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RE: Identification of Historical Populations of Coho Salmon (*Oncorhynchus kisutch*) in the Oregon Coast Evolutionarily Significant Unit

Dear Heather:

Thank you for the opportunity to comment on the above referenced document. I would like to compliment the Technical Review Team (TRT) on producing a clear and readable product. I found the description and analysis used to be transparent and understandable. I found the population characterization (dependent, potentially independent, functionally independent) to be a very useful framework for understanding the populations that comprise the ESU. The analysis that supports the distinctions was very clearly described and not hidden in modeling jargon that I have seen from other TRT products.

The population characterization creates an excellent framework for the development of management strategies for recovery. As the TRT develops recovery goals and strategies it appears that hatchery stocks can be considered as dependent populations. The only substantive comment I have is that Table 2 could be reconstructed as below:

Table 2. Locations and distances of river mouths for basins along the Oregon Coast

9	Basin Name	Latitude	Longitude	Population Type	Distance to Nearest Functionally Independent Population
					(For Dependent and Potentially Independent Populations)

Thank you very much for the opportunity to comment.

Sincerely,

Kenneth F. Bierly
Deputy Director

Subject: Re: TRT Report
Date: Mon, 17 May 2004 14:40:29 -0700
From: peter lawson <peter.w.lawson@noaa.gov>
To: William Percy <wpercy@coas.oregonstate.edu>
CC: Heather Stout <Heather.Stout@noaa.gov>,
Thomas Wainwright <Thomas.Wainwright@noaa.gov>

Hi Pete,

Thanks for sending the hard cc of the report on Identification of historical populations of Oregon coho.

It is well written and I thought an excellent analysis of existing information to assess historical populations and classify stocks into the three categories of independence. Good work by a good team!

I had a few minor comments:

I had the most difficult time, as a ocean type, with the section on Population Classification Results. Specifically the relationship between historical population sizes and proportion of native returns. I think the basis for historical populations, with all its assumptions, is fine, but you assume that the proportion of returns is directly related to population size, which is related to degree of isolation (are we getting circular here?).

Has this been tested?

10 What is the empirical evidence that straying is less for large populations?

Uremovich (1977) found higher straying among broods of chinook poorest overall returns, suggesting the ocean conditions that were related to homestream fidelity. And for coho, Shapovalov and Taft (1954) noted a positive relationship between the amount of straying and the number of outmigrants; they hypothesized that "conditions existing at the time of migration to the ocean determine the amount of straying that will take place one or two years seasons later." This makes intuitive sense if overseeding and density dependent survival occurs for large populations. In any event, I doubt that the relationship between straying and population size is linear!

I think that the team may underestimate the importance of small populations to the long-term adaptability and evolution of OCN coho. You state that small populations may produce poor-quality offspring through loss of genetic diversity, depensation, and inbreeding, hence they have higher probabilities of extinction than large populations. But you also say that small populations may provide reservoirs for adaptive diversity. This is important.

11 My recollections of metapopulation theory, which the TRT buys for coho, harkens back to papers by Sewall Wright in the 40s. He thought that partial isolation of local populations is favorable for new alleles to develop from inbreeding and drift, favoring rapid evolution in a changing environment, Local adaptations are hence the stuff that allow for adaptive radiation and

provide resilience against events such as future climatic changes. So some of your dependent or potentially depend populations may be crucial to long-term viability. As the IMST has said in the past, from the management perspective, don't write off the importance of small coho populations.

12 | On Marine Distribution: Do coho encounter a "warm California Current" when they enter the ocean in the spring? The California Current is that major sluggish offshore current. It is warm relative to subarctic waters and the cool upwelled waters along the coast, and it is the latter that the fish enter. Fig 8 gives the impression that coho enter this broad current up to 100 miles offshore! In reality, few juveniles are caught beyond the shelf break and coastal water influences by upwelling during the spring-fall periods. You could add that during periods of a strong coast jet, some fish entering the ocean in May or June may be advected to the south before they swim to the north.

13 | Also, as I have said several times (see my little book), (Is it in the OSU bookstore?) it is curious that many of the juvenile coho that Hartt and Dell tagged in the Gulf of Alaska in the 50s and early 60s were recaptured or returned as adults to Oregon and Washington before there were large scale hatchery releases, suggesting the wild fish migrated much farther to the north than we suspect they do today.

Bill

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Comments on:

IDENTIFICATION OF HISTORICAL POPULATIONS OF COHO SALMON (*Oncorhynchus kisutch*) IN THE OREGON COAST EVOLUTIONARY SIGNIFICANT UNIT

Clearly historical data are insufficient to give a clear picture of Oregon Coastal Coho productivity and population structure before the recent major habitat modifications, increased harvests, and hatchery programs. Therefore, taking a modeling approach to construct a story about historical coho populations is a reasonable endeavor, provided the story is not given more credibility than deserved. The modeling approach used is a reasonable one, but necessarily is based on some assumptions that are reasonable but not strongly substantiated by experiment or other data. The report does an admirable job of making these assumptions explicit. I have several suggestions of ways to analyze the data that are available and thus refine the story, but am more concerned that the initial assumptions may be wrong in ways that will tend to push the whole recovery planning effort in inappropriate directions.

If this report were merely to be used as an historical basis for numerical recovery targets, the approach would be fine. I am greatly concerned, however, that it will be used as the basis for policy decisions in recovery planning, that are highly dependent on the assumptions used, and that this document will come to be seen as justifying use of those assumptions where they really matter.

The central questions in Oregon Coastal Coho recovery planning have to do with population structure and the extent, depth, and importance of local adaptation. The authors of this document begin by assuming a position on these questions, and proceeding within constraints imposed by that initial position. Specifically, the authors assume that stream mouths are the most important focus in discussions of straying, that straying is governed by an exponentially declining distance function, and that persistence of local populations is a positive function of amount and quality of freshwater habitat. Again, these assumptions are reasonable, but are essentially unproven at the scales used here.

The consequence of the emphasis on stream mouths in straying analyses is to divert attention from the possibilities of intra-basin population subdivision. By assuming that they do not need to look at patterns of fidelity and wandering within basins, the authors have an easier time justifying neglect of local adaptation within basins. Considerable research on other salmonids demonstrates capabilities for rapid population subdivision and local adaptation in the face of considerable genetic interchange. The likely outcomes of such adaptation include higher local carrying capacities and greater production overall, so this issue is relevant to the goals of returning Coho populations to economically important levels. Overall, the analyses of possible intra-basin subdivision appear superficial and driven by a desire to minimize complexity. A few years ago I assembled a list of rather anecdotal examples of morphological and life history differences among Oregon Coastal Coho that lead me to suspect a greater degree of local adaptation within as well as between stream systems than is admitted here.

14

The assumption of an exponentially declining distance function for population exchange is not only not proven, but a fair body of data are available contradicting it. Two examples: in fall 2001 substantial numbers of fin-clipped coho were found in coastal rivers on spawning surveys and at traps, including the one at Siletz Falls. Coded wire tags were found in some of these fish, and indicated they were from Columbia River hatcheries. I have not seen a detailed analysis, but my impression of the raw data was not of a decline-with-distance function. Hatchery Steelhead seem to have a higher stray rate than Coho, and streams without hatchery programs often have 15% or more fin-clipped fish. My impression, from discussions with people catching these fish, is that very often clip patterns indicate these are not fish from the closest streams with hatchery programs. For example, this winter (2004), hatchery Steelhead in the Yachats River were apparently from the south coast, not from the Alsea or Siuslaw.

15

The second assumption (exponentially declining distance function for population exchange among ocean entry points) has implications about the still poorly known ocean navigation and homing systems in salmon. If this assumption is correct and applies at the scale indicated here, then salmon presumably are responding to global cues (magnetic field, elevation of the sun, etc.) rather than primarily to chemosensory clues. A century of hatchery acclimation practice assumes the opposite, and has good enough results to justify continuation. Further, the assumption implies that navigation system failures are essentially de-tuning rather than complete failure. At some level this is probably true (the strays we see have found their way back to the west coast), but I am not confident it applies well enough to validate an exponentially declining distance function.

16

I would like to suggest some alternatives that have at least some support, and that I think need consideration. First, I suggest revisiting the hypothesis that choice of freshwater entry point is influenced by chemosensory clues. This would mean that strays tend to enter streams that “taste” somewhat like their native streams. Obviously, we know little about how freshwater entering the ocean tastes to salmon, but a little speculation may be worthwhile. I believe most of this is ground that has been gone over in the salmon literature, at least speculatively. Underlying geology should provide chemosensory clues. In the context of the ESU, this could mean that streams in basalt areas give different clues than streams in exclusively sedimentary geology, and the Umpqua and Coquille should be really different from the rest. Lakes systems likely have substantially different water chemistry than lake-less streams. Outflow from streams with extensive estuaries undoubtedly has a different chemical signature than outflow from streams without estuaries. And, there likely are chemical signals that are correlated to stream size, at least categorically. Small streams as a group probably lack certain chemical constituents that result from extended weathering or decompositional processes in larger systems. If this chemosensory alternative applies, then straying might tend to be biased toward stream mouths more similar to the source system in whatever chemical clues are most important to the fish. Lake system fish might tend to stray into other lake systems, and fish from small streams into other small streams.

17

Second, incidents such as the widespread straying of Columbia Coho in 2001 suggest a more catastrophic failure of the navigation system. These fish may have lacked the ability to find their source river, and so entered whatever stream was convenient when they were physiologically ready. Or they may have sorted somewhat according to secondary clues (e.g., preferentially picked larger rivers over small creeks).

18

Third (and this suggestion leads to some refinements that can be done to the analysis) the attractiveness of stream mouths to salmon likely varies temporally. The report documents this for some extreme cases (p. 26 for Devil's Lake, Sixes River, New River), but is likely more widespread. In particular, access to many smaller streams is difficult until after the first substantial fall rains, while the larger rivers may be accessible much earlier. I suggest that an analysis can be done to test the effects of this on model results of population dependence by overlaying a function of the overlap in spawn timing graphs (as in Fig. 7) on the exponentially declining distance function. I suspect that this would, for example, move the New River/Floras system from Potentially Independent to Functionally Independent on the basis that the bulk of the Coquille River fish would already be inland before the New River mouth opened. Actually, using spawn-timing graphs would be conservative because in general the fish in large systems spend more time in fresh water before spawning than fish in smaller streams, so their "decision time" leaving the ocean is even earlier.

The third assumption, that persistence of local populations is a positive function of amount and quality of freshwater habitat has to be true at some level, but I would challenge the levels set in this document. I consider the level (capacity to produce 150,000 smolts - vertical line on Figure 20 [new figure number 21]) unreasonably high. Note the text on p. 56 explaining this choice:

"The stochastic life-cycle model (Nickelson and Lawson 1998) produces quantitative extinction probabilities. However these probabilities are sensitive to many of the model parameters including patterns of freshwater production, density dependence, straying, and marine survival. As a consequence we were unwilling to use the absolute extinction probabilities from the model to define the vertical line criterion. We were, however, more comfortable with the qualitative model result; as habitat quantity decreases, extinction probability increases exponentially. We chose as our criterion for persistence, the point where the probability of extinction started to increase rapidly (Fig. 21[new figure number 20])."

19

I understand the authors' reluctance to use the model extinction probabilities. However, the break (inflection) points in the graphs are subject to the same sources of error as the probabilities, and using them instead does not buy any robustness. In my opinion, the model has several properties that makes it unsuitable for this kind of use, either with the probabilities or with the inflection points. First, it undervalues density-dependence. In reasonably good habitat juvenile Coho grow larger before smolting when at low densities, and much hatchery and other research demonstrates that larger smolts have higher ocean survival rates. Second, the freshwater life cycle stage limiting Coho smolt production differs from stream to stream, making total miles of habitat a poor metric of productivity. For example, the Devils Lake system is considered by the model too small to be potentially independent, but the lake itself would have been such good rearing habitat before introduction of bass, etc. that smolt production should have been very high. Third, the model results presented do not consider the buffering effects of

inter-cohort genetic exchange on the effects of poor years on small streams. Very small returns in years of poor ocean survival have a potential for genetic impoverishment in small populations, but inter-cohort exchange, through jacks and four-year-olds can buffer this effect. Devils Lake and other lake systems have appreciable numbers of fish remaining in fresh water an extra year before smolting. This increases probability of persistence in two ways. These older, extra-large smolts should have much higher ocean survival, and their longer life contributes to inter-cohort exchange.

20 | What using the inflection points does, is push the line to the right, qualifying fewer systems as potentially or functionally independent. Actually, the authors have pushed the line even farther to the right than warranted by the inflection points. On Figure 21 [new figure number 20], the marked break points are actually around 15 miles of habitat, rather than the 20 selected for use in this analysis (In a previous version of this analysis Nickelson used 14 miles).

21 | The report as a whole builds an illusion of a population structure in which a few large systems with undivided large populations are key to survival of the ESU, and in which smaller systems lack the capacity to maintain independent populations long enough to evolve significant local adaptations. The policy consequences of this illusion are enormous. This approach will lead to a recovery strategy in which population status in smaller systems is considered not very relevant to overall recovery, or to regulation of ocean harvest. If these assumptions are wrong, the likely consequence is a “recovery” strategy leading to reduction in genetic diversity and less local adaptation, leading to chronically reduced productivity and complete loss of some smaller streams as Coho producers.



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June 2, 2004

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Dear Pete,

The Oregon Department of Fish and Wildlife (ODFW) has reviewed the April 16, 2004 Co-Manager's Draft of "Identification of Historical Populations of Coho Salmon in the Oregon Coast Evolutionarily Significant Unit" and provides the following comments. In general, ODFW approves of the process taken to identify the historical populations and commends the Oregon Northern California Coast Technical Recovery Team (ONCC TRT) for bringing significant new knowledge to the coho recovery planning process. The State of Oregon has already begun using this population structure in the State's assessment of the Oregon Plan for Salmon and Watersheds as it relates to coastal coho salmon.

The Oregon Workgroup of the ONCC TRT has done a laudable job of attempting to identify and classify historical populations of coho salmon along the Oregon coast. The draft document, however, does not describe at any length how the results of these efforts will be used in the recovery planning process. Thus providing the necessary context for the review. ODFW is well aware of the limited information available to assist in this exercise and the need to make reasoned assumptions on historical population structure and interactions. However, because assumptions must be made and it is not clear how these populations will be used, ODFW recommends that the ONCC TRT be cautious in making those assumptions. The ONCC TRT should describe how they envision this information being used in future aspects of this process and seek to ensure that any errors in assumptions do not significantly increase the risk to the recovery and sustainability of the ESU and also do not lead to unreasonable recovery expectations. Areas where ODFW believes the ONCC TRT should review their assumptions and use caution are identified below.

Identification of Populations

22 The identification of historical populations that comprised the Oregon Coast Coho Salmon ESU is one of the more important tasks that the ONCC TRT was charged with undertaking. Populations identified in the document under review will likely be the basis for decisions on ESU viability and recovery. There is the potential within the conceptual model used to identify populations, for some of the historical populations to be overlooked or misidentified. Because a level of certainty cannot be placed on the methods used in identifying populations, caution needs to be taken in defining potential populations. Caution should also be taken in applying the results of any identification method chosen to the definition of a viable and persistent population, and ultimately, a viable and persistent ESU. ODFW recommends that the ONCC TRT consider strategies in upcoming viability analyses that ensure that the potential misidentification of populations will not pose a significant risk to the recovery of the Oregon Coast coho salmon ESU and minimize the potential for unforeseen and unintended consequences.

Classification of Dependent Populations

23 ODFW agrees that it is likely that some populations in smaller basins relied upon immigration from other populations to persist. We believe caution needs to be taken in classifying those populations that were likely to have been dependent populations. Since the desired future status of the ESU will likely be based on historical population dynamics, it is important to categorize each population as accurately as possible. ODFW recommends that the ONCC-TRT describe how they intend to use these classifications and what measures will be taken to re-evaluate their accuracy and importance as new information and analyses become available.

Historical Abundance Estimate

24 ODFW is concerned with the estimated historical abundance of 3.3 million adults described in Appendix III in the Co-Manager's Draft. This estimate appears to represent a best possible modeled historical abundance and is misleading. Overestimating historical abundance for the recovery planning process underway could create unrealistic expectations of what viable and sustainable population levels should be. An overestimate will also suggest that the habitat can be more productive than it may actually be. The estimate is based on maximum production from each population at the same time – an unrealistic situation. ODFW suggests developing a range of abundances based on the approach that not all habitats are productive at any given time. Creating upper and lower limits that capture the uncertainties in the data would be more appropriate. The ONCC-TRT should also describe how this estimate would be used in their future work.

25 ODFW also questions a suggestion made in the Discussion section of Appendix III. In this section the authors suggest that the 30% difference between the intrinsic potential estimate of abundance and the estimate made from cannery records could be evidence that significant habitat destruction occurred at the turn of the century. To use a comparison of two different abundance estimation methods to infer significant habitat loss is just one plausible explanation. There are many possible explanations for the difference in the two estimates; not the least of which is that

one or both of the estimates could be in error. ODFW suggests the ONCC TRT be cautious in speculating about factors for decline before the TRT has completed their analyses into the causes of decline. The success of the recovery planning process depends on the scientific products being accepted by the public. It is important for the ONCC TRT to produce reports for the recovery planning process that are scientifically defensible. ODFW suggests the above-mentioned portion of the Discussion section be expanded to include other possible explanations for the results described.

26

ODFW would like to thank the ONCC TRT for allowing us an opportunity to review and comment on the Co-Manager's Draft. The document is well written and should allow even non-technical readers to understand the concepts and decision processes described in it. The methods used to identify and classify historic populations are well reasoned. The inexact nature of the methods, however, needs to be clearly identified. ODFW suggests it may be useful to describe in this document how the decisions made in the document will be used in subsequent reports and what precautions are being taken to reduce any risk to the viability of the ESU that could result from inadvertently misidentifying populations and their interactions. It will be important to continually remind reviewers how each product of the TRT fits into the overall work of the group, as we currently lack the ability to review the sum of the parts. The ONCC-TRT should also incorporate a feedback loop that can be used throughout their work to look back at previous assumptions and decisions and assess if they are still valid and do not impose unnecessary risk. ODFW staff would be more than happy to meet with the ONCC TRT to discuss our comments if clarification is needed.

Sincerely,

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Responses to Comments

1. The discussion on page 4, paragraph 3 presents the concept that ESUs (also known as Distinct Population Segments, which is the way the ESA is written, but the terminology is confusing) are defined primarily by large genetic divergences operating on time scales of tens to hundreds of generations. Populations are demographic units (measured by survival and reproductive success) where individuals interact at time scales of a few days to a few generations. We presume that the Functionally Independent populations are more similar to each other than to populations in a different ESU.
2. The use of 100 years persistence is seen frequently in the conservation literature as a benchmark. Even though we don't have methods that give differentiation for fine scale differences, for example, between 100 and 90 years, it is useful as a rhetorical tool for visualizing the time scale we are talking about. We are also attempting as much as possible, to be consistent with the McElhaney (2000) Viable Salmonid Population Document.
3. The model used to define the breakpoint includes two sets of ecological rules. At higher densities, a density-dependent production rule dominates. At lower densities, a depensation rule is more important. The breakpoint in Figure 20 (previously Figure 21) shows where the transition between the two rules occurs.

Two recent examples of the effects of small population size in small habitats are Cummins Creek, a direct ocean tributary, and Cascade Creek, an Alsea River tributary. In Cummins Creek, the 1995 brood which produced 730 smolts evidently returned no adult spawners in 1998, as no juveniles were found in summer 1999 and there were only 7 smolts estimated in 2000 (likely leftovers from the 1994 brood). In Cascade Creek, the 1998 brood consisted of 1 male, 7 females, and 7 jacks. This brood produced only 13 smolts.

4. See the discussion on page 54, paragraph 2. Changing the native return rate to 90% or 99% simply changes the position of points on the y-axis without changing the relative position of the populations.
5. We did perform sensitivity analysis on these values and found the results that were discussed in response #4.
6. We performed a sensitivity analyses by scaling migration probabilities to the degree of overlap in migration timing and found that the results were not sensitive to that change. We also did a + or - 50% on the arithmetic scale to see if that affected the vertical (Independence) line. Only a few populations were affected. We have revised Figure 19 (previously Figure 20). to include the +/- 50% lines. As we continue to develop the viability criteria, we will continue to reexamine the location of the "independence line."

The highest conservation risk occurs if there are more populations, and more independent populations, than we define. In this case we would underestimate the number of

populations needed for a viable ESU. In developing our viability criteria we recognize this risk and will be stipulating that in each historical population as defined in this document there be a distribution of spawners throughout the population. This recognizes that the populations we have defined are large enough to have considerable substructure, and this substructure is important to population viability.

There is also the risk of defining too many populations. In this case we could impose an unnecessary burden on regulatory agencies, resource use, and development. In our view the Independent populations we have defined are about the largest divisions on the Oregon Coast that are consistent with a conservation goal. Also, remember that we are defining historical population, not the present situation.

7. We agree with your comment: the smolt traps are in substantially different locations within the basins. There may be a substantial migration from trap to estuary during which growth can occur. Also, these fish may be hanging out in the estuary to gain weight and size before they make the transition to the ocean.
8. The figures of spawn timing are from the year 2001, which was chosen as a somewhat “normal” year with no substantial drought, El Niño, or excessively low adult abundance.
9. See Table 8, a new table to present this information in a more readable format.
10. We have revised the Homing fidelity section, on page 36.
11. Other TRTs have practically ignored smaller populations. Our scheme, which defines everything with an independent ocean entry point as a population, was developed specifically because we felt these small populations were important to the functioning of the ESU. We will revisit the role of small populations in our viability criteria and analysis. This may result in changes to our historical population discussion as well.
12. We have deleted Figure 8.
13. We have revised the Marine distribution section, based upon your comments. See page 33.
14. We draw a distinction in the report between migration (movement between basins) and straying (movement within a basin). We have reviewed the CWT data for evidence of migration patterns of Columbia River and coastal hatchery fish. Migration of hatchery fish between basins tends to be strongly centered around the basin of origin (see text in Homing fidelity section, page 36).
15. We did not postulate whether global or chemosensory cues were responsible for the migration patterns we modeled.
16. These ideas about the nature of the “taste” of basins are very interesting and worthy of study, but as far as we are aware, are untested.

17. We do not consider straying or migration as necessarily a “failure of the navigation system” but, rather an adaptation to variable environmental conditions.
18. We used spawn timing as an additional isolating mechanism in several of our isolation analyses. The results were not greatly influenced. In particular, New River/Floras did not change status.
19. Perhaps we were not clear on how the models were used. The Nickelson – Lawson model was used to estimate the 15 miles of habitat as a cutoff for 100-year independence. At low population levels, egg-to-parr survival is at a maximum of 44%. The curve is driven by compensatory factors related to the difficulty of finding mates at low densities. The data used to determine independence came from an analysis of the Intrinsic Potential of each system to produce coho salmon. Lake areas were incorporated in this calculation. This was subsequently input into the Relative Independence Model described in Bjorkstedt 2004.

Although smolts at low densities sometimes show faster growth rates and higher marine survival rates, and these are certainly of biological interest, the effects are probably not important at the resolution of modeling we are doing here.

20. The value used in the analysis was 15 miles. We have corrected the text.
21. We appreciate that the reviewer is concerned that we are undervaluing small populations. In fact, we consider them to be important to the health of the ESU and very probably in genetic divergence of some populations, but they play a different role from the large populations. We are at this time building the theoretical framework for including both large and small systems in our viability definition. Our assessment of historical coho salmon habitats may be revised as we proceed with our viability analysis.
22. This document does not attempt to define current populations, or what future populations should look like. It is our intention that recovery will specify restoration of processes that will enable fish to establish populations in whatever configuration suits them – not necessarily what has existed in the past. This document is an attempt to define how historical populations worked, but we are not locked into the populations that are defined here. Current and future populations will probably look different and be dealt with during the viability analysis.
23. We recognize that populations are not necessarily monolithic units and they may have internal structure, even though we may not know what that structure is (see response 6). Therefore there may be the need for spawning aggregations within populations. The future status of the ESU will not necessarily be based on historical population dynamics. The historical populations defined here are being used only as a template of how Oregon Coast coho salmon populations operated before their structure was so significantly affected by European fisheries and land use practices. This definition and classification of historical populations is not an implementation plan, it is only the first step to define how we believe historical populations worked, so we can utilize that in our viability analysis. Again, it is the intent of recovery to restore the ability of populations to

function properly. In the viability report we will reevaluate the dependent/independent classifications and current population genetics should be available for use in the next report.

24. Production estimates are higher than those observed in recent times due to the inclusion of habitat that has not been in production for 100 years. Much of this lowland habitat was diked, filled, or ditched for agricultural purposes early in the process of European settlement, but was an integral part of historical Oregon Coast coho salmon population dynamics. See Appendix III for an expanded discussion of this analysis.
25. Historical abundance of coho salmon was estimated for the purpose of modeling the effects of relative abundance on population independence. See Appendix III for an expanded discussion of this analysis.
26. It is our intention to revisit our conclusions with every new analysis and not be constrained by our original definition of historical populations, or even our original definition of viability. This is intended to be a feedback loop where if our original definitions or classifications don't make sense in the context of the next steps, we will change them. We look forward to working with ODFW staff throughout the TRT process to produce useful, scientifically defensible reports that contribute to our understanding of recovery and what that means for Oregon Coast coho salmon. See also response #6.